Introduction

Stephan Boehm, Christian Gehrke, Heinz D. Kurz and Richard Sturn

PROGRESS AND CRITIQUE

'Systematic progress in philosophy is nothing but remembering what oneself has said.' Many economists will find this statement by the German philosopher Hegel puzzling at best. If applied to economics, it is likely to be deemed abstruse rather than useful. Yet some reflection shows that this statement casts some light on the situation of historians of economic thought. Discussing and scrutinizing systems of knowledge, theories or particular models in terms of progress is inevitably linked to some idea of critique, that is, to the ability to give some account of the scope of intellectual achievements promoted by these models, theories or systems of knowledge.

Hegel's statement invokes a broad idea of critique not necessarily attached to an evaluative stance. Such an open concept of critique is useful for understanding radical differences between different styles in the history of ideas and may summarize the common ground between quite different approaches to the latter. For instance, it encompasses rational reconstruction as the attempt to clarify the truth of conjectures and the scope of explanatory sketches by means of analytical models. This may be thought of as a positive aspect of critique leading to progress in knowing what exactly can be stated as positive results and what remains a more or less plausible conjecture. This positive critique is essential for a diagnosis of success or failure in terms of a given theory's own explanatory or normative purposes. Hence it is crucial for assessing whether in some historical period progress occurred in the sense of unfolding the potential of a particular theory, or whether some theorist was a precursor of a certain theoretical development (whether or not (s)he played a role in triggering this development). Put in a more general way, critique is crucial for the idea of progress *within* certain intellectual traditions or within particular theoretical discourses. As John Chipman once put it, 'What makes the history of economic thought interesting is the study of how truth comes out of controversy'.

The general idea of critique also entails negative dimensions making clear what *cannot be said* relying solely on the models, theories and systems of knowledge. This type of critique helps us to discriminate between different types of failure. Does a theory fail because of internal reasons, that is, the inconsistency of its assumptions or axioms? Or does it fail because of some external reason, because aspects are systematically neglected by this particular theory, or else because these aspects are just incidentally left out, or ruled out, by some capricious set of assumptions? Negative critique sometimes has the potential to develop new horizons: the negative style of critique is often linked to arguments which show, or purport to show, why it would be interesting to be able to say something precisely on those matters which are beyond the scope of the theory under consideration in its present state. Philippe Mongin's account of the development of *normative economics* comes close to the conception of a dialectics of progress along the lines just sketched, where some qualified diagnosis of failure (such as Arrow's impossibility theorem) is the crucial starting point for new theoretical developments.

Perhaps the style of positive critique is most promising with respect to the set of particular models which operate against a common background of assumptions and definitions and a common pre-analytic vision. With respect to such theoretical contexts, it may make sense to say that a new theory is better than an old one if and only if it explains everything which was explained by the old one and something more. Limiting oneself to positive critique is problematic with regard to whole systems of knowledge, for example with respect to epistemic systems as understood by the French philosopher Michel Foucault. It is impossible when different systems of knowledge are related to each other in a way which, in the tradition of Isaiah Berlin, could be called 'irreducibly pluralistic'. Pluralism of this kind may make the very idea of theoretical progress obsolete, much in the same way as it arguably fails to make sense to ask whether Goethe is superior to Shakespeare or whether Shakespeare is superior to Homer.

At first sight, the question 'Is there progress in economics?' seems to be challenging *only if* progress, in one sense or another, is understood as an evaluative concept. For if 'progress' is just a synonym for any kind of change, if it lacks any connotation of improvement or development, the answer seems to be trivial. Something is always changing, if only the number of active professional economists or of university departments and journals in economics.

Klaus Hamberger's paper shows that the diagnosis of progress does not become trivial even if its evaluative meaning is set aside. There may be disagreement regarding the assessment of when, how and why a shift of epistemic ground occurs. Hence it *may* make sense to approach the question of change *without* the evaluative stance invoked by the word 'progress', provided the author focuses on the relevant aspects of change. A non-evaluative notion of 'progress' may thus be found suitable for the history of systems of knowledge. Taking this insight on board, it becomes almost inevitable to ask the question which Donald Winch has chosen as the title of his chapter: 'Does progress matter?' Perhaps it does *not* matter because it would make sense only if used in a weak, non-evaluative sense. It may well be the case that the more demanding, evaluative conception of progress rather than the more trivial one renders the question 'Is there progress in economics?' meaningless. In this more demanding interpretation, an answer to the question presupposes at least some common epistemic ground. If economics is taken seriously as a practical subject, it must even presuppose common ethical ground. But is this requirement not something that should be avoided, given the degree of heterogeneity of the different schools of economic thought and the heterogeneity of moral credos? Some believe these days that it should be avoided at all costs. Yet to push this point too far carries with it the risk of being self-defeating, at least if one sticks to the idea that historians of economic thought are in the business of putting forward true propositions concerning systems of knowledge. This seems to imply that there *is* some minimal common ground because otherwise there is no basis for settling such truth-claims.

There is another reason for rejecting sweeping dismissive attitudes with respect to evaluative interpretations of progress in economics. Of course, many problems with evaluative conceptions of progress do not emerge in theoretical contexts, which are clearly limited in one way or another. Several chapters in this volume devoted to developments in particular fields, such as monetary economics or trade theory, or particular currents of thought, such as Austrian economics, or particular analytical issues within a certain framework, such as modern classical economics, provide ample evidence of the usefulness of a more demanding conception of progress in the history of ideas. Mongin's short history of normative economics shows how productive positive and negative critique *from within* may be. Mark Blaug's contribution is a vivid example that even the problems linked to the heterogeneity of explanatory purposes and analytical frameworks across different strands and styles of thought can be coped with. Blaug's critique of Walrasian general equilibrium theory shows that meaningful evaluative critique need not be from within but may get its momentum from a confrontation with problems which are not internal problems of the theory. Finally, Sergio Cremaschi studies the role of metaphors in Smith's *Wealth*. He rejects the post-modernist view that the use of rhetorical devices *ipso facto* proves that there is no truth and theoretical progress in economics. On the contrary, it was not least Smith's judicious choice of new metaphors which allowed him to widen the scope of economic explanation.

SOME METHODOLOGICAL ISSUES

If it were not for its teleological, linear overtones one might be tempted to use *progression* for 'progress' in the aforementioned evaluative sense. Indeed, in order for ‘progress’ to make any evaluative sense, there has to be some sort of progression, an accumulation of knowledge handed down from generation to generation rather than the perennial replacement of those who had in turn replaced their progenitors. It is in this loose sense that progress in economics is closer to the sciences than to the arts or humanities. It may be instructive to consider the starkly different situation in philosophy in order to dispel any lingering doubts about the status of economics. The history of philosophical thought may be said to exhibit strong discontinuities, that is to say, progress in philosophy seems to consider itself as a complete break with the past, as a new beginning or clean slate, involving a repudiation, even negation, of the past, including all the numerous earlier new beginnings which at the time had also been held to provide the subject with secure foundation for years to come. It seems to be a mark of the really great philosopher to claim that *everything* that had gone on before rested on a fundamental mistake and had therefore to be rejected *in toto*. And this is one way to keep the past alive: by the need of those who wish to place the subject on a new footing to disassociate themselves from their progenitors by some monumental destruction exercise. Thus conceived, the history of philosophy emerges as nothing but a succession of bold visionaries helping us to liberate ourselves from the nightmares of the past. So instead of something being passed on from one generation of scholars to another, an accumulated body of knowledge, there is a continual replacement of those who had in turn replaced their forerunners. Previous stages in the history of the subject are not stages of discovery of what we now know, nor is the present a stage of discovery of what will eventually be known. There is no systematic cognitive development; what went before is rather like a bad dream from which we are liberated by the arrival on the scene of the great figures.

Consider the situation in economics. Notwithstanding faint echoes of progress being aided by the current generation of incumbents passing away, can anybody seriously claim that any of the revolutionary economists who are usually admitted to the pantheon of the history of the field, be it Smith, Marx or Keynes, made their respective contributions by jettisoning everything that had gone on before? To pose the question is to answer it.

That ‘progress’ in economics is an essentially contested concept on which there is bound to be widespread disagreement emerges as a common thread uniting the chapters assembled in this volume. Roger E. Backhouse refers to progress as being ‘a very complex phenomenon’. (The ‘complexities of a contestable concept’ also figure in Uskali Mäki’s contribution.) Backhouse points to a distinction that he has made in earlier work between *theoretical* and *empirical* progress. Blaug takes over the distinction and applies it to contemporary economics, and concludes that contemporary economics clearly exhibits theoretical progress, while it is found lacking with respect to empirical progress. Historical analysis is necessary to ascertain what conceptions of progress could be used to analyse past economists' work without imposing on them criteria that they would not have understood. Judgements about progress might have to be local, restricted to limited periods of history; they are not meant to be absolute, but this would be good enough for most purposes. The distinction between *local* as opposed to *overall*, *global* progress seems to be highly pertinent in economics.

Another dimension along which progress can be delineated relates to the unification and expansion of its domain. Uskali Mäki, in particular, raises the issue of economics imperialism and scientific advance. He notes several of the ways in which economics imperialism might be construed (i) as progress by conquest, and (ii) as conducive to generate progress. Luigi Pasinetti, on the other hand, points out that a commonly recognized process does not exist in economics whereby scientific progress can unambiguously be ascertained. Owing to the particular characteristics of economics, scientific advance may frequently be described by the co-existence of competing – and sometimes complementary – research traditions, or 'paradigms'. Co-existence should be seen as a sign of vitality rather than of immaturity; and discussions across paradigm boundaries should be encouraged rather than suppressed.

Jack Birner points to the discovery that what was thought to be the truth is neither the whole truth nor nothing but the truth as an important engine of progress in science. This mechanism, christened 'the correspondence principle' by Karl Popper, says that science advances by replacing a theory that was believed to be complete and true by incorporating it into another theory, which explains why the previous theory was incomplete and wrong, why those who held it thought it to be true, and in addition explains phenomena the previous theory did not explain. This mechanism has been further analysed by Wladislaw Krajewski, Noretta Koertge and Heinz Post, who called it 'conservative induction'. Birner contends that the twentieth century has seen several episodes in which economists made use of the correspondence principle, probably unaware of this philosophical literature.

Erich Streissler's provocative contribution, culminating in the sweeping claim that as far as economic content is concerned, '[t]here has been little which is new in macroeconomics since the early heyday of our science in the eighteenth century', is bound to raise some eyebrows. What is particularly noteworthy here is that Streissler's striking thesis amounts to some kind of 'reverse whiggism', or 'anti-whiggism', in sharp contrast to the prevalent (among non-historians) condescending Whig interpretation of the history of economic thought ('Whig fallacy', 'presentism'). It is the chief characteristic of the latter view that it reads history backwards, interpreting and evaluating the achievements of the past in terms of the prevalent standards, criteria and questions of the present.

THE PROBLEM OF PROGRESS IN SELECTED FIELDS OF ANALYSIS

Whether progress or regress has occurred in certain sub-fields of economics is investigated in a number of contributions focusing attention on four different fields: normative economics, monetary economics, trade and location theory, and modern classical economics. In addition, there is a set of individual contributions, each of which discusses progressive (or regressive) elements in the historical development of different currents of thought, ranging from conceptualizations of the circular flow in physiocratic and post-physiocratic writers to the parallel treatment of information costs, deliberation costs and transaction costs in modern microeconomic theory.

Two papers are concerned with developments in monetary economics. Hans-Michael Trautwein provides an assessment of the credit theory of Carl Knies, which has not received much attention outside the German-speaking countries. He argues that a close examination of Knies' failure to integrate money and credit into the framework of value theory is instructive in exposing the fundamental difficulties of modern attempts to integrate intrinsically useless money into the framework of general equilibrium theory. Augusto Graziani's paper on 'New lines of research in monetary economics' emphasizes that while in Irving Fisher money performs essentially the role of a means of payment, in John Maynard Keynes the emphasis is on money as a stock of wealth. According to Graziani, the Keynesian concept has several analytical advantages; nevertheless the Fisherian concept has seen a revival in more recent times. Moreover, Graziani argues that limiting the analysis to positions of full equilibrium neglects important features of a monetary economy. As a promising new line of development, he then turns to more recent contributions of the so-called 'circulation approach' to the theory of money, in which money is contemplated both in its role as a flow (means of circulation) and a stock (money balances as a stock of wealth).

Four papers take up the theme of trade and location theory. Andrea Maneschi examines what is known as 'new trade theory', which was developed over the past two decades. He asks what the novelties of this paradigm are relative to conventional Heckscher–Ohlin–Samuelson trade theory. His answer is that there was indeed progress, but much less than some of the practitioners of the new paradigm maintain, because many of the findings had been anticipated by authors such as Bertil Ohlin. Sergio Parrinello focuses attention on the institutional factor in the theories of international trade, old and new. He argues that the new trade theory has the merit of dealing explicitly with this factor, but that the way it does so is problematic: the institutional factor cannot be reduced to the single notion of a public good. He then turns to the question of how the old trade theories could incorporate that factor; special emphasis is placed on the non-Walrasian approach. Stephen J. Meardon deals with an offspring of new trade theory, the 'new economic geography', and scrutinizes Paul Krugman's claim that the new economic geography is able to capture long-known ideas which previously fell flat because they could not be expressed in terms of a mathematical general equilibrium model. Meardon argues that, alas! Krugman's criteria of progress are self-referential and his assessment therefore unconvincing. However, in terms of the explicit criteria of progress put forward by scholars such as Ohlin, Lösch and Myrdal, the new economic geography represents qualified progress. Manuel Fernández López presents two cases of independent discoveries or anticipations of important doctrines or analytical tools: one concerns spatial economics, where Johann Heinrich von Thünen has been said to be the 'founding God' (Samuelson); the other concerns linear programming, which is closely associated with the name of George Dantzig. López attempts to establish the fact that the two were anticipated in important respects by two Spanish scholars: Thünen by the engineer Pedro Antonio Cerviño (1757–1816); and Dantzig by José Barral-Souto (1903–76).

Richard van den Berg surveys contemporary responses to François Quesnay's *Tableau économique*. He shows that its usefulness as an analytical tool was not appreciated for a considerable time and that it took several generations of economists before further refinements and new applications were forthcoming. Sandye Gloria-Palermo attempts to identify, at the level of methodological and conceptual considerations, progressive and regressive elements in the development of Austrian economics from Carl Menger's original contribution to those of modern Austrians. Hansjörg Klausinger tells the story of the development of a consistent general equilibrium framework for stock-flow analysis in the IS–LM model as an example of progress in macroeconomics and then shows that the insights gained from this formulation seem to have been lost again. He illustrates his thesis by referring to three examples of 'regress': the confusion over the meaning of the flow equilibrium condition of the market for bonds; the interpretation of the liquidity trap; and the validity of Poole's rule. Maurice Lagueux argues that the extension of the notions of information costs, deliberation costs and transaction costs to non-market domains is bound to make the concept of rational choice vacuous.

The papers by Fabio Ravagnani, Heinz D. Kurz and Neri Salvadori, and Pierangelo Garegnani deal with the meaning and scope of the long-period method as it was developed by classical economists from Adam Smith to Ricardo and revived by Piero Sraffa. Ravagnani argues that the normal prices determined in a classical framework of the analysis are not limited to economies characterized by stationary or quasi-stationary conditions. Such prices may on the contrary be considered as useful theoretical approximations of observable magnitudes even outside such conditions. In their comment, Kurz and Salvadori stress that the long-period method is an indispensable tool of analysis, but that certain important aspects of real economic systems require this tool to be complemented by others in order to achieve a better understanding of the problems at hand. Garegnani recalls the fact that the abandonment of the long-period method by marginalist authors was motivated, first and foremost, not by an attempt to render the theory more 'realistic', but by the difficulties the long-period demand-and-supply theory encountered because of its deficient concept of capital. He contends that a formalised dynamic *theory* is in conflict with the premises of classical theory; this does not preclude, however, the formulation of dynamic economic *models* which help to explore the implications of special assumptions.

A FINAL WORD

The 'Whig interpretation' focuses attention on those lines of development in the past – to the exlusion of all others – which culminate, or seem to culminate, in present arrangements. Therefore, it is by definition that historians of economic thought are 'anti-Whiggish'. It is testimony to the seriousness of the contributions to this volume that estimations on the price of progress, musings on 'the world we have lost' or revelling in the pastness of the past have not been on the agenda. What may be recorded, though, as one of the overriding impressions emerging from the present volume is the relevance, indeed the indispensability, of the history of economic thought for contemporary cases of theory evaluation. The views documented here are a striking antidote to the notion that the historical origin and development of a doctrine or research tradition are irrelevant to its rational acceptability.